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zone entre 35° et 40° N.; Paris Observatory, Carte photographique du ciel, Zone 24 (24 Charts), presented by the Observatories; S. de Glasenapp, Mesures micrométriques d'étoiles doubles; H. L. Rice, Theory and Practice of Interpolation; G. V. Schiaparelli, Osservazioni del pianeta Marte, 1888, presented by the authors; Photographischer Mond-Atlas, Heft 7, 8, presented by Prof. Weinek; Photograph of a meteor (lantern slide and enlargement), presented by C. P. Butler; Bronze copy of the Jubilee Medal of the Royal Meteorological Society, presented by the Society.

On Stationary Radiants of Meteors. Reply to the Criticisms of M. Th. Brédikhine. By H. H. Turner, M.A., F.R.S., Savilian Professor.

In *Monthly Notices, R.A.S.*, vol. lix. p. 140 (1899 January), I suggested an explanation of the stationary or long-enduring radiant points which Mr. W. F. Denning has so long and faithfully declared to be the outcome of his observations. In the *Bulletin de l'Acad. Imp. des Sciences de St. Pétersbourg* for January last (T. xii. No. 1), M. Th. Brédikhine has done me the honour to examine my suggestion at some length, though he ultimately dismisses it as leading to a totally different result from that which it endeavours to explain. He declares that I have forgotten to take account of the fact that the Earth's path is curved, and that my explanation would only hold good if the Earth moved in a straight line. I hope, however, to show that the mistake is his own, and that my suggested explanation remains untouched.

My suggestion was briefly this. Suppose we have originally a swarm of meteors crossing the Earth's orbit at a point N_0. The action of the Earth in passing again and again through the swarm will cause some of the meteors to describe new orbits crossing the Earth's orbit at a point N_1 displaced from N_0 in the direction opposite to that of the Earth's motion, but without altering the average *relative* motion of the Earth and meteors. Some of these, again, will be caused to describe new paths crossing the Earth's orbit at N_2, and so on, so that ultimately, instead of the point N_0, we have a long series of points N_0 N_1 N_2.

In the ordinary language of meteoric theory the point N is called the node, and it is a quite familiar idea that the node should be made to regrede in this way by the perturbations of the Earth. I merely avoid using this familiar language because there are two unfamiliar ideas on which I wish to insist which are liable to be overlooked if we think of the motion of the node as it would occur for a planet. These points are:

First, that the crossing-point of the swarm does not move as a whole; for some members of the swarm it remains unaffected, for

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others it moves rapidly, and for yet others it moves with intermediate velocities, so that instead of a single crossing-point we ultimately get a series distributed round the orbit. Hence instead of the familiar retrogression of the node we have a *distribution* of the crossing-point along the orbit. I have given the reasons for this view in my paper above quoted, and as M. Brédikhine does not criticise them, I do not reproduce them here.

Secondly (the main point of my paper), the velocity of the meteors at the crossing point *relative to the Earth* remains unchanged during the displacement or distribution. Now, for this perfectly definite geometrical notion M. Brédikhine has substituted a completely different one, as follows : Suppose we draw from the crossing-point N a line NR to represent the relative velocity of the meteors in magnitude and direction, and a line NA to represent the absolute velocity or velocity in space, and let NE represent the velocity of the Earth. My principle (reasons for which are given in my paper at some length) is that NR remains constant in length and direction. Hence NA, which is obtained by combining NR with NE, varies both in length and direction as N moves round the orbit. There is no difficulty in tracing these changes if this be desired ; but it is clear that the simplest account of what happens is given in terms of NR and not of NA. Nothing could be simpler than that NR remains constant.

M. Brédikhine, however, fixes his attention upon NA, the velocity in space. He assumes that NA remains constant *in length* and in *direction relative to NE*. In fact he assumes that as N rotates round the orbit it carries with it the two lines NE and NA as though they were rigidly attached to the radius SN from the Sun to node. Naturally, therefore, the diagonal NR of the parallelogram formed by NA and NE rotates with them, and is anything but fixed in direction. There is no need to prove this by a page of spherical trigonometry (p. 114 of M. Brédikhine's paper), and it may save others wading through an argument which would have been made easier by a figure if I point at once to the place where M. Brédikhine has made this wholly unwarranted assumption, viz. at the top of p. 115 of his paper, where he writes

$$l'=l-5^\circ, \quad b'=b.$$

Here l and b are the longitude and latitude of the velocity of the meteors in space when the node is in a position N, and l' b' are the longitude and latitude when N has moved through 5°. This shows that he treats NA as rigidly attached to the radius vector SN (or the direction of the Earth's motion NE, which amounts to the same thing) in *direction* ; that he treats it as constant in magnitude is less obvious, but appears from the derivation of the coordinates (λ' β') for the relative velocity, *i.e.* for the radiant. He even takes the trouble to convert longitude and latitude into R.A. and Decl., as though that might affect matters !

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Now, I cannot but think that this complete misconception of my meaning arises from regarding the phenomenon to which I wished to draw attention as a motion of the node, with the associations which this familiar idea calls up. M. Brédikhine has visualised the actual orbit of the meteors as being simply rotated about the Sun; this is very far indeed from the conception I wish to put before astronomers. It may be that my conception is not deducible from facts, though I feel more and more convinced of its truth myself; but at any rate, if true, it leads at once to a stationary radiant—there ought not to be the least difficulty in admitting so much. If, on the other hand, M. Brédikhine's conception is deducible from facts, I should be the first to acknowledge that a radiant cannot be stationary, without any calculations at all; but I cannot see that anything he has said in his paper supports his view in the least degree. His investigation in the earlier part of the paper as to what happens to a meteor after a pair of encounters with the Earth supports, so far as it goes, either my view or his own: he proves that a small motion of the node will result; but he proves nothing conclusive as to the changes in velocity either real or apparent; indeed, he never examines the velocity of the meteor at all—he merely *assumes* that the velocity in space remains sensibly parabolic, and this assumption I regard not only as unproved but as quite erroneous.

As the principles I have advocated are thus apparently liable to be misunderstood, I will venture to repeat in somewhat different form my main argument.

If the Earth were at rest, and a meteor passed near it, its path would be deflected. The whole effect of the Earth's action may be summed up by considering the state of affairs when the meteor enters the "sphere of influence" of the Earth and when it leaves it.

(*a*) The magnitude of the velocity at entering and leaving is the same.

(*b*) But the direction is altered, being deflected in a plane through the Earth's centre.

(*c*) The time of passing through the sphere of influence is shortened; that is, if the time which would be spent within this sphere if the Earth were annihilated be T, then, when the Earth is replaced, the time will be $T-t$; because the velocity of the meteor is always increased by the Earth's attraction and never diminished.

Now, by assuming a *pair of* passages through the sphere of influence symmetrically disposed on opposite sides of the Earth we have

(*a*) The magnitude of the velocity still unchanged.

(*b*) The deflections being equal and opposite in the two cases neutralise each other, and the total deflection is zero. Hence the velocity is now the same on leaving the sphere *in magnitude and direction* as on entering it. [As regards the magnitude,

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however, there may be a diminution of the kind suggested at the end of this paper.]

(*c*) The total time spent within the sphere of influence is still further shortened. Hence, let us disregard entirely what happens while the meteor is within the sphere of influence; suppose, for instance, that we entirely lose sight of it, and only know the following facts:—

It enters with a certain velocity.

If there is no Earth inside the sphere it emerges with the same velocity after time T.

If there *is* an Earth inside it emerges still with the same velocity, but after a shorter interval, T—*t*.

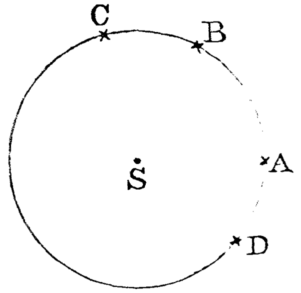
Now, what is true for the actual motion of the meteors when the Earth is at rest is true for the relative motion when the Earth and meteors are both in movement and both acted on by the same forces, as, for instance, the attraction of the Sun. The attraction of the Sun on Earth and meteors while the latter are within the sphere of influence of the Earth is sensibly the same; the difference is the "disturbing force" of the Sun familiar in lunar theory, very small even for the Moon, and very much smaller for meteors passing close to the Earth—in fact, quite negligible.

Hence, the meteors obey the laws above stated for their relative motion, viz.—

They ultimately emerge from the sphere of influence with the same velocity *relative to the Earth* with which they entered it.

This happens whether the Earth is existent within the sphere or is supposed annihilated; the only difference is that in the former case the meteors emerge after a time T—*t*, in the latter after a longer interval T.

Now, remark that these laws hold *independently of the time spent within the sphere of influence.* We cannot extend this time indefinitely, for that would mean that the meteor would describe a complicated orbit near the Earth; but we can suppose it extended to such a length that the Earth's path is sensibly curved during the period. Let, then, T be a sensible fraction of a year, so that the Earth's path can no longer be taken as straight, but is sensibly curved. [M. Brédikhine has suggested that I have been misled by considering only a small straight portion of the path. I think he is mistaken.]



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Let, then, T be such a time that during it the Earth describes an arc AC (much exaggerated in the figure) of its orbit round the Sun S; and let the meteors enter the sphere of influence at A. If we suppose the mass of the Earth annihilated they will emerge again with the same relative velocity at C. The effect of the Sun's attraction has been to make both Earth and meteors describe the curved path AC during the interval, but has not disturbed their relative motion, which remains unchanged throughout.

We need not suppose the Earth annihilated, but merely that it does not happen to be at A when the meteors come. The sphere of influence can still be described, but it is to be regarded as empty. This may happen many times, and the meteors will continue to enter at A and emerge at C, their orbit forming a sort of long contact with that of the Earth.

But there will come a time (the periods of Earth and meteors being incommensurable) when some meteors arrive at A to find the sphere of influence with an Earth inside it. The effect will be that they will be ejected earlier than before, say at B; but still with relative velocity unaltered (if we are allowed to combine the effect of a pair of symmetrical encounters into one). Their orbit will at the next return enter the sphere of influence at D and leave it at B, where AD = BC;* and unless another encounter with the Earth occurs, the orbit will be permanently shifted round through the arc CB, or AD. But the velocity of the meteors *in space* at D is not the same as the *velocity in space* was at A either in magnitude or direction; nor is it obtainable from the velocity at A by rotation round S as M. Brédikhine suggests: the arc AC is subject to such a simple rotation and becomes the arc DB, but to find the new velocities in space at D and B we must combine with the *unchanged relative* velocity, the changed velocities of the Earth.

The constancy of the relative velocity is in fact maintained quite independently of what happens to both Earth and meteors during the time spent in the sphere of influence so long as both are affected equally. Suppose, for instance, that a giant comet were to rush by during this time and cause a considerable change in the orbits of both Earth and meteors. The relative motion at parting would still be unchanged, which means that these meteors would still come from the same radiant point as before.

May I now consider a weakness in this theory of the distribution of the crossing-point round the Earth's orbit which was pointed out by Professor S. Newcomb, who very kindly discussed it with me at some length during his visit to Oxford last year? I have

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* This is easily seen by considering all the motions reversed when the meteors are about to leave the sphere of influence. In the first case, if they are sent back again at C they emerge after time T at A. If sent back again at B, and if now the Earth be absent from the sphere of influence, they will similarly emerge again after the same time T at D, where CA = BD, or CB = AD. The orbit of the Earth is of course here regarded as circular.

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thought over his criticism many times since, and I think it may be answered. It may be stated as follows: Admitting for the sake of argument that the perturbing action of the Earth on a meteor swarm is of the kind described in my paper and briefly reproduced above, it is so small at any one encounter that a large number of encounters and a long period of time are necessary for any sensible distribution of the crossing-point. Now, the general perturbations exercised by the other planets on the orbit of the meteor swarm will cause it to slowly change, so that those meteors which passed close to the Earth at one time will gradually pass further and further from it, new ones taking their place. The action required will thus never accumulate to sensible proportions.

My answer to this very pertinent objection is as follows:—

In the first place, if we assume that the velocities of meteors are parabolic, their velocities relative to the Earth are so large, and they rush through the sphere of influence so quickly, that the effect I have indicated is certainly small. I have shown how small it is in my previous paper. But are we very clear as to the size of these velocities? Our knowledge of meteoric velocities is based on very scanty information. Mr. Denning is of opinion that it is worth very little, and he has a right to an opinion if anyone. Photography may ultimately help us in this respect; but meanwhile it will be well not to be too sure that the relative velocities of meteors and Earth may not be comparatively small. There is no reason why some meteors should not be describing orbits rather like that of the Earth; indeed, there are many reasons why they should. If the relative velocity is small the distribution of the crossing-point may be effected with much greater rapidity.

Secondly (and here I make the suggestion with some diffidence, but yet I think it worth making), is it not possible that the Earth has an attractive effect on the orbits of meteors passing near it, so that those which would otherwise be carried away by planetary perturbation are retained, and perhaps even those which originally passed wide of the Earth's orbit are drawn towards intersection with it? If this were the case it would explain several things—not only would it complete the above theory of stationary radiants without any difficulty, but it would explain the large number of meteors encountered by the Earth without assuming that all space was full of meteor swarms.

To show how such an action might be possible, take the case with which I originally started, of two meteors, AC, BD, passing on opposite sides of an Earth, E, supposed at rest. Their paths are deflected towards each other equally and in opposite directions; so that, if everything were exactly symmetrical, the meteors would collide after the encounter. If they were perfectly inelastic they would then proceed together along the central line FG with diminished velocity, viz. the resolved component of the deflected velocities in the direction FG.

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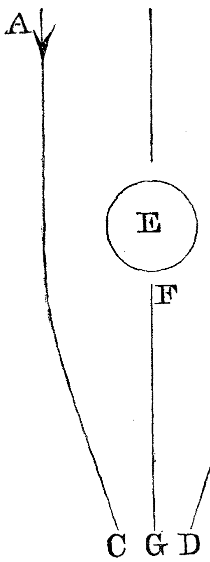
Now, to imagine two single meteors meeting in collision in this way is almost ridiculous. But when we consider a whole swarm of meteors originally travelling in parallel lines, then there will be after the passage near the Earth a considerable convergence of the swarm towards the line FG, which will almost certainly result in a number of collisions or entanglements, the general effect of which would be (except in the improbable case of the meteors behaving like perfectly elastic particles), that some of the velocities perpendicular to FG, the general direction of motion, would be destroyed. The convergence is symmetrical with respect to the line FG (other things being equal), and for inelastic particles we can imagine a complete destruction of velocity perpendicular to FG, so that all the paths then coincided with FG. At the next return the meteors so affected would then all pass through the position occupied by the Earth's centre. If the Earth were still there, they would of course fall on the Earth and be seen as meteors; if not they would pass through this point on the Earth's orbit instead of all passing wide of it as before; and this is exactly the sort of action above suggested—a tendency to draw the points in which the meteor orbits cut the ecliptic nearer to the actual path of the Earth. To suppose the whole swarm symmetrically distributed round the Earth, and the subsequent collisions to resemble those of inelastic particles, is of course an extreme supposition; but it is one towards which the actualities must approximate in some degree, and hence cause is shown for a tendency in the direction indicated.



We may regard the matter from another and more familiar standpoint. When we see in the sky a nebula with a nucleus we do not find any difficulty in imagining that the nebu-

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lous matter is condensing towards the nucleus; indeed, it is the natural supposition. It would be still perfectly natural if the nucleus, instead of being permanently in place, were to appear there only at comparatively rare intervals, being annihilated in the meantime. The condensation would proceed more slowly, but would proceed nevertheless. If, again, the nucleus made these visits by actually passing through the nebulous mass periodically along a certain line, then the condensation would be towards this line generally, and not towards any particular point of it. If, further, the path of the visiting body were not a fixed line, but any one of a series of lines forming a surface, the condensation would be towards this surface.

Now, this is surely just what is happening with a meteor swarm. To a spectator carried with the swarm, and unconscious of its motions in space, the Earth appears as a periodical visitor passing through the swarm. Its path always lies on a certain surface, viz. the surface described in the swarm by the Earth's orbit whenever the swarm crosses this orbit. Hence the swarm will gradually condense towards this surface, and so *tend* to become a sheet which passes through the Earth's orbit, or rather through which the Earth's orbit sweeps at successive returns. This tendency may not be very strong, owing to the rarity of the Earth's visits, but I do not see why it should not be stronger than the perturbing influence of the other planets, considering their remoteness compared with the proximity of the Earth. Such considerations would only apply to swarms which originally passed near the Earth. Those in the neighbourhood of other planets would be similarly collected by them into sheets passing through *their* orbits; and in the case of the giant planets, like *Jupiter*, the condensation would be much greater. Could these condensations of meteor swarms ultimately form comets? Have we here a clue to the established connection of comets and meteors, and to the association of periodical comets with the major planets?

That such general speculations are hazardous I am only too well aware. I put them forward with due apologies. My main point is this. We have the statement of a thoroughly experienced observer that certain meteor radiants are stationary or persistent. That this observer knows what he is saying is evident from the fact that he distinguishes other radiants which are *not* stationary; and, further, there are tests which may be applied to his observations generally which have not yet shown them to be otherwise than excellent. Starting, then, from this observed fact, can we suggest an explanation of it? M. Brédikhine's "explanation," of course, does not count, as it is a simple denial of the main fact. Mr. Denning claims the physical connection of certain radiants appearing in the same part of the sky in different months; M. Brédikhine denies the physical connection, and attributes the connection to chance, and in this he has a large following. Is there any good reason for this scep-

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ticism or positive disbelief until possible causes for the fact have been examined? I have endeavoured to indicate a possible cause in the simple attraction of the Earth. The considerations I have adduced are not very thoroughly established, but they point in the right direction, and do not demand any new supposition. Further, they are not at variance with known results of more elaborate mathematical investigations; and that they are not capable of being fully confirmed by, or compared with, such results at once is due to the fact that mathematical investigations have not yet been carried far enough. The perturbations of a planet on a swarm of meteors considered as a whole have been examined, but not the selective action on different parts of the swarm, and certainly not the interaction of differently affected parts. Until this has been done, why should we deny the possibility of the facts which Mr. Denning claims as observed facts?

Observations of the Leonids made at the Cambridge Observatory on 1899 *November* 13, 14, 15. By Arthur R. Hinks, M.A.

1. The observations to be discussed were made by a number of members of the University who had volunteered to assist in securing as complete a record as possible of the expected Leonid shower. My acknowledgments are due, first, to Mr. John C. W. Herschel, B.A., Research Student in Astronomy, St. John's College, who has taken a large part in the preparations, observations, and discussion of the results; and, secondly, to the following gentlemen: Messrs. J. F. Cameron, B.A., Fellow of Caius College; L. E. H. R. Barker, B.A., A. L. Hall, B.A., and H. E. Wimperis, Caius College; R. W. H. T. Hudson, B.A., A. B. Field, and M. Walker, St. John's College; W. E. Hartley, B.A., and G. W. Walker, B.A., Trinity College; F. M. Oldham, B.A., Trinity Hall.

We attempted three things:—

I. A continuous count of all meteors seen (five observers).
II. Records of trails from visual observations (two observers and a timekeeper).
III. Records of trails photographically (three observers).

The weather was on the whole not unfavourable, though we suffered from a thick haze late on the night of the 14th, when the shower was least feeble.

I. Horary numbers of observed Leonids.

2. Four observers watched continuously on the nights of the 14th and 15th, dividing the sky among them; and a fifth recorded every five minutes the counts of the four. The numbers